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Research Paper

An experimental manipulation of divisibility in the public good game ⋆

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ABSTRACT

We present the results from two framed field experiments in which members of real communities that share naturally occurring public goods can voluntarily contribute to a common pool which, after being increased by the experimenter, is either divided in individual vouchers (as in standard public good games) to be used for buying private goods, or paid out in group vouchers to be used to buy public goods for the community. We find that contributions are larger in this second case, and when individuals belong to the same community. Furthermore, contributions in our naturally occurring communities tend to grow over time. Finally, contributions in the standard public good game are strongly correlated with contributions to the naturally occurring public good. These results add to the growing literature that investigates the external validity of public good games.

1. Introduction

Many modern–day economic challenges, from the difficulty in fighting climate change and infectious diseases (Buchholz and Sandler, 2021) to the crumbling infrastructures of many market economies (Petroski, 2017), revolve around the underprovision of public goods. For these reasons the study of public goods is so important and social scientists have extensively utilized lab experiments

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based on the public good game (PGG) to explore the intricacies of public good provision in real world scenarios (Ostrom, 2000; Chaudhuri, 2011). Given the reliance of this literature on the PGG framework, questions about its external validity have recurrently surfaced and have been empirically investigated (Galizzi and Navarro-Martinez, 2019).

The baseline PGG follows an invariable script: first, each subject in a randomly formed group is endowed with a private good in the form of money or tokens; then each subject decides independently how much of this private endowment to transfer to a common pool; at this point the experimenter increases the common pool according to a known multiplier, so as to mimic scale effects characterizing naturally occurring public goods, and finally the augmented common pool is distributed among the subjects, usually following an equal split. In PGGs, subjects are strangers to each other, they begin and end the game with a private endowment and any change in utility comes from a change in their private payoffs.

Let us compare this to the voluntary provision of a naturally occurring public good[1] (NOPG) where subjects are often members of a community (i.e. teammates involved in a project, village members sharing a well, fishermen in a lake) which may either precede the existence of the public good, or even be defined by it. These subjects own private endowments out of which they have to decide how much to contribute to the public good. To the extent that the public good is produced, they enjoy a combination of the utility from their private consumption (if any) and the utility of sharing the consumption of the public good which, however, remains a common and indivisible resource of the community. It is because of their *indivisibility* that such goods are public/collective (Sutter, 1996; Cornes and Sandler, 1996; Olson and Zeckhauser, 1966). Conversely, dividing a good in shares (as it happens in the PGG) means transforming each share in a *private* excludable goods.[2]

While the size of the group can range from small circles (as in the case of a team) to large groups (such as fishermen of a large lake) to the whole of humankind (as in the case of climate change), the existence of *communities* is another key condition for the existence of most NOPGs. However, natural communities are usually absent in laboratory experiments.[3] Typical experimental subjects are anonymous strangers who cannot collectively consume indivisible public goods, so researchers take the shortcut of augmenting and splitting into equal shares the common pool among participants under the implicit assumption, based on induced-value theory (Smith, 1976), that individual monetary payoffs accurately reflect the strategic incentives of corresponding NOPGs. This design choice implicitly assumes that each share of the augmented common pool generates, for each individual, a utility equivalent to that generated by the joint consumption of a public good that could be bought with the common pool.

Note that this is an assumption and this paper explores precisely this point: is individual willingness to contribute to a common pool the same, if the common pool is split among group members to buy a private good for each of them as in a PGG (we call this situation a *Divisible* common pool), or if it is used to buy a public good to be shared by all group members as in a NOPG (i.e., an *Indivisible* common pool)? We designed our experiments to address this specific question.

Several papers and an extensive meta-analysis (see the literature review in Section 2) have addressed the external validity of PGGs by comparing the behavior of individuals in a standard PGG and in naturally occurring settings. While our paper squares neatly in this literature, its novelty is in the precise manipulation of the divisibility dimension of public goods across otherwise identical experimental conditions.

In our two framed field experiments, implemented with scout groups, we combine features of PGGs and NOPGs. We build on a linear voluntary contribution mechanism (VCM) in which contributions to a common pool are first augmented by a factor of 1.5 and then paid out in vouchers either to individuals (*Divisible*) or to communities (*Indivisible*). The design is such that, for given contributions, total payoffs are the same in the two conditions. Still, they are split into individual shares in one case and collectively assigned to the entire community in the other. The communities we analyze are scout patrols[4], i.e., naturally occurring groups, including on average five people, that regularly raise funds to finance the collective purchase of indivisible goods such as tents and stoves. The extent to which each patrol member can benefit from such goods is independent of the individual contribution to their acquisition (non-excludability). These characteristics make the patrol goods in all respect public goods as it is non-excludability that traditionally distinguishes *public* goods from *club* goods.[5]

In the design of our first experiment, we manipulate (i) the divisibility of the augmented common pool and (ii) group membership. Treatment (i) is implemented by paying subjects with either individual vouchers (*Divisible* condition) or collective vouchers (*Indivisible* condition) to be spent at the local scouting equipment store where both individual equipment (e.g., boots, uniforms, backpacks) and patrol equipment (e.g., tents, stoves, woodworking tools) can be bought.

[1] Following the literature (Laury and Taylor, 2008), we use the term *naturally occurring* public good to describe both public goods that occur "in the wild" (i.e. are not set up by an experimenter), as well as to public goods that are *artificially* created by an experimenter but are *natural* insofar as they involve an indivisible good similar to those typically owned by the community under analysis.

[2] The property of *indivisibility* is conceptually different from the one of *non rivalry*. For example, a village well is indivisible - it cannot be divided among users in advance - whether water is plentiful (non rival) or scarce (rival). Shared access must still be managed in both cases.

[3] Group membership is sometimes manipulated or primed in PGGs (see later) but artificial groups can hardly co–own goods.

[4] Scout groups, also known as troops, are organized into patrols: small teams of five to nine members. Each troop typically consists of two to six patrols. All troop members share a common social framework (the Scout Promise and Law), a similar age range (12-16), and a strong sense of identity and collaboration. Each patrol operates autonomously, both cooperating and competing with the other patrols in the troop, and is responsible for acquiring and managing collective materials and resources such as tents, stoves and other equipment. Scout patrols share social norms and involve identified individual roles within the patrol; no wonder these naturally occurring small societies have attracted the interest of scholars in different disciplines (see the literature review in Section 2).

[5] Clearly patrol goods are excludable to those outside the patrol. However, what matters for our analysis is their non-excludability and indivisibility *within* the patrol.

Treatment (ii) allows us to dig deeper into the role of the underlying community. The two subjects matched in the VCM could belong either to the same patrol (*Same* condition) or to different patrols (*Other* condition), and the potential value of the common pool is the same in the two cases. In our two–by–two factorial design, we have a naturally occurring public good only at the intersection between the *Same* and *Indivisible* conditions. At the intersection between the *Other* and *Divisible* conditions, we have a standard PGG. Results from our first experiment show that contributions to the common pool are higher when individuals are paired with a member of their same scout patrol than when they are paired with a member of a different scout patrol. Contributions to the common pool are also higher when the common pool is paid indivisibly to the scout patrol compared to when it is divided between individuals; furthermore, individual choices are strongly correlated across conditions.

By keeping the size of the group fixed at two subjects, the design of our first experiment has the advantage of allowing us to disentangle the effects of both indivisibility and community membership. At the same time, the fixed number of players introduces a potential confound in the payoffs between the *Divisible* and *Indivisible* conditions. While in the *Divisible* condition, the two subjects who can potentially contribute are also the same who benefit from the division of the common pool, in the *Indivisible* condition, this is no longer the case. Indeed, the potential contributors are still two, but the beneficiaries of the indivisible good are all patrol members. The larger number of beneficiaries in the *Indivisible* condition might exacerbate the free-riding problem, ultimately affecting our results.

To address this limit we designed a second experiment which differs from the first under three main aspects. First, the reference group always coincides with a scout patrol, and we only manipulate the divisibility of the common pool, so that the number of beneficiaries under the *Divisible* and *Indivisible* conditions is the same. Hence, the *Divisible* condition works as a PGG between patrol members. In contrast, in the *Indivisible* condition, the augmented value of the common pool is given to the patrol itself, and the individual payoff only includes the amount not contributed to the common pool. Second, we moved from a *one-shot* to a *repeated* game where individuals have information about past behavior of their peers. In doing this, we are following another crucial characteristic of many naturally occurring public goods, that require recurring contributions to their maintenance. We are confronted with a well–established line of research in the experimental literature that has shown chiefly that contributions to public goods tend to wane over time (Ostrom, 2000). Third, we forgo the community membership treatment explored in the first experiment[6] as all groups playing the VCM always belong to the *Same* patrol.

In line with the results of the first experiment, we find that subjects' contributions to the common pool are higher in the *Indivisible* condition compared to the *Divisible* one. Concerning the dynamics of contributions over time, as opposed to the rather standard result that contributions to PGGs tend to decrease over time, we find that as the experiment progresses, contributions increase and even more so in the *Indivisible* condition.

The remainder of this paper is organized as follows: Section 2 briefly presents the existing literature, Section 3 introduces a theoretical model, Section 4 presents the experimental design and the procedures implemented in both framed field experiments, Section 5 presents the results, Section 6 discusses some of our design choices while Section 7 concludes.

2. Related literature

Within the very large literature on PGGs, this work naturally relates to a number of other papers that focus on studying their external validity. There are mainly two approaches followed in the literature.

A first group of papers moves from the standard PGG setup with subjects who do not belong to a natural community and goes on to check how these subjects play in another follow–up NOPG that presents a public good social dilemma, such as planting shade trees in an urban area in the United States (Laury and Taylor, 2008); contributing to a university course group assignment in Austria (Reindl et al., 2019); working on a team data entry task in Germany (Englmaier and Gebhardt, 2016); or contributing to a community project in Ethiopia (Voors et al., 2012). Stoop et al. (2012) run another interesting experiment with recreational fishermen randomly grouped in teams of four at a privately owned fishing facility in the Netherlands; the experimenters artificially created a social dilemma by providing non-excludable cash incentives to these fishermen to catch fewer fish. Although fishermen are paramount examples of communities who experience free-riding challenges in the management of their commons, in this specific case these fishermen are strangers, and property rights and excludability typically allow recreational fishing centers to consume their fishing stock optimally. In this group of papers, the same subjects, who are strangers, play two experiments: a context–free PGG experiment and a NOPG experiment involving a more ecologically valid public good — which, however, is not necessarily a good the subjects would have naturally chosen to contribute to if it were not for the experiment.

Another group of studies takes the opposite approach and starts with subjects belonging to communities already characterized by the production of NOPGs such as fishermen working in areas at risk of over-fishing in Brazil (Fehr and Leibbrandt, 2011) and Colombia (Torres-Guevara and Schlüter, 2016); user groups participating to a forest commons management program in Etiopia (Rustagi et al., 2010) and in Nepal (Bluffstone et al., 2020); and a village divided by a river deciding about the construction of a bridge in Vietnam (Carlsson et al., 2014). The experimenters then run a PGG with some subjects of these communities to study whether the observed naturally occurring behavior correlates with the behavior in the PGG. In this group of papers the contribution to the NOPG is never experimentally manipulated but only observed and sometimes only at group level. Our paper is closer to this last group inasmuch as our subjects belong to a natural community; however we also experimentally manipulate the contribution to the *Indivisible* common pool.

[6] We do not examine this second dimension both because the results from the first experiment are already conclusive and because the implementation is virtually impossible given the variable size of the group, as explained in Section 4.

Interestingly, most of these studies (Laury and Taylor, 2008; Reindl et al., 2019; Englmaier and Gebhardt, 2016; Fehr and Leibbrandt, 2011; Rustagi et al., 2010; Bluffstone et al., 2020; Carlsson et al., 2014) reveal some degree of correlation between cooperative behavior in the PGG and in the NOPG, with some notable exceptions (Voors et al., 2012; Torres-Guevara and Schlüter, 2016).

In a major effort to assess the external validity of lab experiments testing social preferences (including but not limited to the PGG), Galizzi and Navarro-Martinez (2019) run both a meta-analysis and an original lab-in-the field data collection. Their meta-analysis shows that only about 40 % of studies find significant correlations, with an overall average correlation of 0.14. Even among significant results, the correlation is modest (0.27), indicating generally weak support for a strong connection between lab and field behavior.

Galizzi and Navarro-Martinez (2019) attribute these disappointing results to the common practice in the literature of comparing behavior in a single social preference game, such as a PGG, with one or more loosely related field behaviors, such as earning or spending money, shrimp fishing, voting in school elections, or registering books in a library (these would correspond to our NOPG). The correspondence between behavior in the PGG and these field activities is often weak or ambiguous, which opens the door to ex-post rationalizations where many forms of pro-social behavior can be framed as relevant to the behavior in the PGG.

Notice that all these experiments have in common the fact that the lab PGG and the related field tasks with which correlation is studied are in the end different activities and therefore it is challenging to determine whether *intrinsic nature* of the public good plays a significant role. Factors such as stake size, participant interactions, and the involvement of groups could also be influential.

While our experiment shares the same goal — studying the external validity — we approach the task in a different way as we maintain the same design in every aspect except for the divisibility of the common pool and the membership to the community, which allows for a different type of comparison.

Alfano and Marwell (1980)[7] is arguably the work closer to ours, as it manipulates explicitly the dimension of divisibility.[8] In this early work the authors ran a public good game with incoming students who would be residents of the same dormitory floor the following semester. They hypothesized a lower contribution to public goods when these are indivisible (each individual having to share the decision concerning its consumption with all other group members), but found the opposite result. Notice that Alfano and Marwell (1980) recruited experimental subjects with no prior interactions, hence solving the problem of interdependencies but having the possibility to run the experiment on just two groups (one per treatment), in a between–subject design that did not allow for a comparison at the subject level. Moreover, subjects in the "indivisible" treatment – but not subjects in the "divisible" one – were informed that they would meet again (in order to determine the use of the money). As the authors acknowledge, this might have looked like an occasion to "*pressure them to disclose their investment decisions*", or to *"establish esprit de corps"* (pg 307) which was not provided in the other treatment. In our case, no such information was needed, as all groups are used to meet regularly regardless of our experiment. Indeed, patrol members have a history, and a future, of regular interactions for the provision of their public goods; this long–lasting time horizon also sets naturally occurring public goods apart from PGGs, which are typically contained in a single experimental session of at most a couple of hours. In fact a rather stylized result of the literature is that contributions to PGGs tend to decrease over time, mainly due to the vanishing relevance of reputational concerns (Andreoni, 1988; Ostrom, 2000). This is usually related to the "no future" nature of artificial groups created in lab experiments between anonymous participants, and is in contrast with the typical nature of public goods (Gächter and Fehr, 1999). In our experiment, instead, patrol members acquire and manage indivisible goods and regularly interact over a long time frame.

Our paper also relates to a small literature that have exploited the patrol–based organization of scout groups as an observation unit: see, for instance, early contributions in sociology (Hare, 1952; Kelley and Volkart, 1952), development psychology (Sharp et al., 2011), and behavioral economics (Battiston et al., 2021). To the best of our knowledge, no previous experimental research has looked at social dilemmas within this, or similarly organized, populations.

Finally, this paper also contributes to the literature on young individuals' behavior in PGGs (Zelmer, 2003; Harbaugh and Krause, 2000).

3. Theoretical framework

Our starting point is a standard VCM to transfer resources to a common pool. Each individual in a group of N members is endowed with w_i and contributes x_i to the common pool. The common pool is augmented by the experimenter by a factor α with $\alpha > 1$. It is then subdivided among the N members, who each benefit from a portion $\frac{1}{N}$ of the augmented common pool. The overall utility of subject i is thus

$$u_i(x_i) = w_i - x_i + \frac{\alpha}{N} G \tag{1}$$

where $G := \sum_{i=1}^{N} x_i$ is the contribution of all subjects to the common pool, and $\frac{\alpha}{N} G$ describes the individual utility resulting from a total investment of G in the public good.[9] The marginal per capita return (MPCR) from contributing to the public good is then simply $u_i'(x_i) + 1 = \frac{\alpha}{N}$ (the fact that it is independent from x_i being an immediate consequence of the linearity of the mechanism).

[7] Note that the same experiment was presented also in Marwell and Ames (1981).

[8] Other papers have used indivisible payments but have not focused on the design choice of indivisibility *per se*: see, among others, Bohm (1972), Milinski et al. (2008).

[9] The normalization of α by N and the assumption that the individual utility from G is proportional to the total investment are made for simplicity of exposition; our analysis is compatible with any strictly increasing function.

Subjects sub–optimally contribute to the common pool whenever:

1. $\alpha \in (1, N)$, as $\alpha > 1$ guarantees that contribution to the common pool is socially efficient, but $\alpha < N$ guarantees that the MPCR is lower than 1 and hence contributing to the common pool is a dominated strategy (Isaac and Walker, 1988); and
2. individuals are sufficiently selfish as to not internalize other group members' utility.

Analyzing a NOPG means taking into account *intrinsic* characteristics of the good itself, and of its procurement. We denote these effects with η so that Eq. (1) becomes:

$$u_i(x_i) = w_i - x_i + \frac{\alpha\eta}{N}G. \tag{2}$$

Whether η is larger or smaller than 1 is *a priori* unknown. Specifically, a single collective investment in a public good will typically be more efficient than a set of individual investments (e.g., economies of scale in the production or purchase of the good): this *scale effect* will drive η upwards. On the other hand, the shared ownership of a good affects the utility for its users: this *joint consumption* effect, including the need to share decisions about the procurement and use of the good, may make it *less* desirable to its owners (as compared to one entirely owned), driving η downwards. Clearly, in standard PGGs $\eta = 1$ (but $\alpha > 1$).

Following the large literature on altruism, self regarding preferences (Charness and Rabin, 2002) and warm glove (Andreoni et al., 2010) in PGGs, we enrich Eq. (2) by including an altruistic component:[10]

$$u_i(x_i) = w_i - x_i + \frac{\alpha\eta}{N}G(1 + (N-1)\beta) \tag{3}$$

where $\beta > 0$ means that each subject values positively the contribution of the public good to the utility of another participant: individuals will want to contribute to the public good if $\frac{\alpha\eta}{N}(1 + (N-1)\beta) > 1$. In particular, if $\beta = 1$, then $\frac{\alpha\eta}{N}(1 + (N-1)\beta) = \alpha\eta$, each individual is internalizing the full social welfare, and there will be no under–provision.

In light of the large literature on in–group preferences (Goette et al., 2006), we can expect that β is larger when a subject is matched with individuals from one's own community, resulting in larger contributions. Our model then predicts that this holds both when contributions go to the indivisible and to the divisible common pool. This is the first testable hypothesis of the model.

In our experiment, we compare the standard PGG design ($\alpha > 1$, $\eta = 1$) with a treatment in which, for the same value of α, G represents the actual provision of a NOPG. This allows us to assess whether behavior is consistent with $\eta > 1$-that is, whether individuals show a greater willingness to contribute when their effort supports a real-world good rather than an abstract one. This distinction is important because it speaks to the perceived attractiveness of scale effects in naturally occurring settings, over and above standard altruistic motives. In naturally occurring environments, however, η is typically confounded with other parameters such as β, making it difficult to isolate its influence on behavior. Instead, in our experiment, this distinction can be done cleanly: both the PGG and the NOPG conditions can involve altruistic motivations, but what makes them different is *uniquely* η. This allows us to test more directly whether $\eta \neq 1$, i.e., whether individuals place additional value on contributing to a good that benefits from positive spillovers due to shared ownership, or, vice–versa, on investing in exclusively owned goods.[11]

Our model purposely does not consider *reciprocity*, and more in general any way for decisions of subjects to influence each other: a higher contribution by a peer will increase u_i, but it will not affect the choice of x_i. This is a harmless assumption as long as we focus on a one shot, simultaneous game, where each player must independently decide x_i without information about the choice of peers. However, our second experiment involves a repeated game, where each player observes the group members' average contribution at previous rounds. It is hence natural to re–interpret Eq. (3) in a context in which subjects obtain information on peers' past contributions. This will not affect α, which is an exogenous parameter of the design. It can instead affect β, which we now assume to embed not just altruism, but also reciprocity — which is indistinguishable from an increase in altruism conditional on peers' contributing (Keser and Van Winden, 2000). It can also affect η — reflecting a preference for owning indivisible goods together with other subjects who also contributed to them, resulting in the following utility function:

$$u_{i,t}(x_{i,t}) = w_i - x_{i,t} + \frac{\alpha\eta(G_{t-1})}{N}G(1 + (N-1)\beta(G_{t-1})) \tag{4}$$

where $\eta(\cdot)$ and $\beta(\cdot)$ are now functions of contributions in the previous round G_{t-1}. For ease of notation we denote with G_0 a reference value that subjects derive from past experience outside the experiment. As mentioned above, the literature on reciprocity (Sugden, 1984; Keser and Van Winden, 2000) suggests that $\beta' > 0$. We are not aware of contributions from the literature that could help us estimate η, but the intuition presented for β suggests that, if any, η should also be positively related to past contributions ($\eta' \geq 0$): like reciprocity, willingness to own shared goods includes an element of trust that peers' contributions can reinforce.

Hence, the model suggests two further testable hypotheses.

First, to the extent that communities are characterized by a higher degree of cooperation (see our first hypothesis above), they should also feature no, or reduced, decay of contributions over time. To see why, consider that a larger G_1 reverberates in larger values of β and/or η, which contributes to a larger G_2, and so on.

[10] The distinction between pure and impure altruism (Andreoni, 1990) is beyond the scope of the present paper, and of the experiment we implemented. Given that utility is transferred to other members by virtue of a voluntary contribution, β will in principle include both. It could even include an element of social norm (Kimbrough and Vostroknutov, 2016).

[11] We further elaborate on this point in the Section 6.

Second, whatever the effect of indivisibility is, the absolute size of the effect should increase over time. Indeed, if at any period, G_t is larger with indivisible than with divisible goods, then this, combined with $\eta(G_t) > 1$, will push towards a further increase of G_{t+1} with indivisible goods, as compared to divisible goods: vice-versa if $\eta < 1$. That is, whatever the effect of indivisibility of the common pool is, we should expect it to be *amplified* over time.

4. Experimental design and procedures

Our data are gathered from two framed field experiments, the first conducted with paper and pencil and the second conducted online, both involving Italian boy and girl scouts. Each scout group (a troop) is divided into patrols; these are organized subgroups that perform activities together. Each patrol is equipped with public goods such as pots, crew pickets, field lamps, tents, stoves, woodworking tools, and other camping equipment. At the same time, each scout needs some private goods such as a backpack, boots, and a uniform to perform the activities (see more details about scouts in footnote 4).

In both experiments payoffs were handed out with both *individual* and *patrol* vouchers to be redeemed to acquire either private or patrol goods at the local scout store (more discussion on the use of vouchers in Section 6).

4.1. Indivisibility and group membership

In the first experiment, we implemented two treatments, with two conditions each, in a two by two within–subject factorial design involving randomly formed pairs of subjects.[12] The first treatment concerned the Divisible/Indivisible nature of the good. Each subject was endowed with €5 and had to decide how much of this initial endowment to put into a common pool. In the *Divisible* condition, the value of the common pool was then multiplied by the experimenter by 1.5 and the result was evenly divided among the two subjects participating in the game. Subjects' payoffs were the sum they kept for themselves, plus the sum of the two individual contributions, multiplied by 1.5 and divided by two, as in a standard PGG. In the second condition (*Indivisible*), the content of the common pool was again multiplied by 1.5, but it was then evenly divided between the *patrols* of the two subjects, rather than between the subjects themselves. The individual payoff was therefore given only by the amount subjects kept for themselves, although subjects also jointly benefited from the undivided common pool.

The second treatment dimension concerned group membership: the two individuals may have been members of the *Same* patrol or of different patrols (*Other*). Both treatments were implemented within–subject – that is, all subjects made four separate choices in the following way: first they were instructed under the *Divisible* (*Indivisible*) condition[13] and they had to declare their contributions both i) in case they were paired with a member of their *Same* patrol and ii) they were matched with a member of the *Other* patrol. After this, they were instructed under the *Indivisible* (*Divisible*) condition and again they had to declare their contributions depending on their pairing with both iii) a patrol mate and iv) a member of another patrol. Whether the *Divisible* or *Indivisible* condition would determine the actual payoff was randomly determined at the end of the experiment, when subjects would also get to know whether they had been paired with a member of their patrol, or of another patrol.

All patrols present at the summer camps took part in the activity simultaneously and in the same area; hence, each of them knew who the other participants were. However, decisions were taken individually and anonymously. Hence, anonymity in our experiment was lower than in most PGG experiments, where strangers do not know each other, but larger than in experiments where the identity of specific contributors is revealed (Chaudhuri, 2011; Samek and Sheremeta, 2014; Gaudeul and Giannetti, 2017).

Given this experimental design, a NOPG is obtained at the intersection of the *Indivisible* and *Same* conditions: this is where subjects know that the entire amount of the common pool will go to their own patrol, hence allowing the subject's community to purchase an indivisible good. By keeping the number of contributors fixed to 2, the first experiment allows us to investigate in conjunction both the divisibility of the good and the membership to a community. Specifically, it allows to test the *Same* vs. *Other* dimension with both a NOPG, and with a divisible common pool such as that typically used in PGGs. While fixing the number of players in the first experiment allows us to explore the community membership treatment neatly, at the same time, it introduces a potential confound as the number of subjects that benefit from the common pool may differ in the two conditions. In fact, there are two contributors and two beneficiaries in the *Divisible* condition, but two contributors and several more beneficiaries (all members of the patrol) in the *Indivisible* condition. A larger number of potential recipients in the Indivisible condition might increase the incentives to free-ride.

4.2. Group real size and dynamics

Compared to the first, the main novelty of the second experiment is that subjects did not play in fixed pairs but rather all members of any given patrol played together regardless of the size of the patrol.

This allows us to compare contributions under the *Divisible* and *Indivisible* condition keeping the number of *contributors* and of *recipients* of the common pool the same. At the same time, this design forced us to sacrifice the *Other* dimension, which would have required forming fake patrols of a varying size to mimic the composition of actual patrols but with each member coming from a different patrol; which is impossible given the typical size and number of patrols inside a troop.

Another novelty of this second experiment is the repeated design, which allows us to investigate the dynamic of contributions. Each patrol member played the same game over a series of ten rounds; after each round, each subject received feedback on the group's

[12] The implications of the use of the within subject design are discussed at length in Section 6 also in light of the results.

[13] The order of presentation of the *Divisible* and *Indivisible* conditions was randomized at the session level (see Appendix A.1).

average contribution. In any round, each individual had to decide how much to contribute in both the *Divisible* and the *Indivisible* condition.[14] Both the condition and the round determining the final payoff were randomly chosen at the end of each session.

Notice that members of the patrol who happened to be absent at the experimental sessions were still going to benefit from the indivisible goods acquired by their patrol; e.g., if the patrol bought a tent, they would not be excluded from its use. Hence, in this second experiment, to preserve comparability between the two conditions, contributions in the *Divisible* condition were also split among all registered members of the patrols, even if they were absent at the session. During the experiment each participant was informed about the total number of patrol members involved including those who were absent, and hence did not contribute.

4.3. Procedures

For the first experiment, seven (in-presence, paper-and-pencil) sessions were conducted during the summer camp of scout troops from Trentino-South Tyrol, a region in Northeast Italy. Each session involved one troop, composed by 4 to 6 patrols, for a total of 31 patrols, and 160 subjects between 11 and 16 years old (average age 14.4 years), evenly distributed by gender (51 % males and 49 % females), for a total of 640 observations.[15] One pilot was run previously in another troop, and one last session was discarded because the summer camp involved only two patrols that were formed *ad hoc* and did not reflect the actual patrols operating during the year. After the explanation of the instruction by the experimenters, the experiment took place followed by a questionnaire and a debriefing.[16] Subjects earned on average €5.11 from this experiment; the 22 patrols that obtained a patrol payment (that is, in sessions where the PGG condition was randomly extracted) obtained on average €14.75 each.

The second experiment, run online, involved scouts belonging to 17 troops from Lazio, a region in Central Italy. Each session involved one troop, but two sessions could not be completed due to logistical problems, resulting in 15 valid sessions.[17] Because of the restrictions of the COVID–19 pandemic, the scout troops were connected online, using the Zoom platform, while they took part to the experimental session implemented via Otree (Buso et al., 2021; Chen et al., 2016). Microphones were muted by default, while participants were able to see each other. Before the COVID–19 pandemic, each scout troop used to meet on a regular basis. During the pandemic, however, their meetings took place online and our experiment was indeed conducted during those online meetings. After the explanation of the instruction by the experimenters, the experiment took place followed by a questionnaire[18] and a debriefing. Each experimental session lasted about two hours. Each troop was composed by, on average, 2.93 patrols (minimum 2, maximum 4), for a total of 44 patrols, and 237 participants. Of these, however, 12 did not provide any useful observation (typically abandoning the session before the first round), reducing the sample to 225 subjects, of age between 10 and 16 (average 13.2 years), not evenly distributed by gender (36 % males and 64 % females). Each subject participated in 10 repetitions of the game, each including two decisions, for a total of 2250 observations for each condition that reduce to 2223 because of 27 missing decisions.[19] Subjects earned on average €4.52 in sessions with individual payments and €1.26 in sessions with group payments; the latter averaged to €26.68 per patrol.

Before the experimental session, subjects were required to answer a comprehension question: given the hypothetical contributions of three members of a patrol, they had to compute their resulting payoffs. Subjects needed to enter the correct answer in order to gain access to the experiment. At any stage, subjects were able to ask clarifying questions in a private chat with the experimenters.

This second experimental protocol received approval by the LUMSA Ethical Committee. We obtained the informed consent of participants' parents and conducted the experiment in accordance with, and under the supervision of, the "*Centro Studi Scout*" of FSE (*Associazione Italiana Guide e Scouts d'Europa Cattolici*), the scout association with which we collaborated.

In both experiments, after selecting a specific region of Italy for a matter of convenience, we invited all groups in the region to participate, and accepted all applicants for which a feasible date was found.

Table 1 displays stylized facts about each experiment.

[14] The placement on screen (right or left) of the two conditions was randomized at the individual level).

[15] Authorization to perform the experiment was obtained in advance from scoutmasters, who are responsible for the choice of activities conducted during the summer camp having received such responsibility directly from the youths' parents. At the time this experiment was carried out (2017), neither the funding organization nor our universities required ethical approval, nor was pre-registration standard practice.

[16] The experimental session lasted about two hours, including another experiment (Battiston et al., 2021) held before the one presented in this paper. Payments were cumulated across the two experiments. Individuals knew since the beginning that they would perform two activities, but the rules for the second activity were explained only once the first one was concluded. Subjects were told their cumulated gains, and paid with the vouchers, only at the end of the entire session.

[17] Participants were provided, in advance, via their troop and patrol leaders with unique codes allowing them to enter the experiment website and allowing us to reconstruct patrols membership. In one session, several pairs of participants were provided identical codes. While the session was run until completion, it was impossible to reconstruct actual contributions and show participants and their group members correct past contributions. In the other session, only 7 participants (out of the 15 expected) connected to the session, so it was interrupted immediately. All descriptive statistics reported in what follows are computed discarding these two sessions. The experiment was pre–registered on No. 25 2020 (https://osf.io/syuvn).

[18] The full questionnaire, including the exact wording of the questions used as covariates in the models, is provided in the appendix of our related paper (Battiston et al., 2021).

[19] If, after a reasonable amount of time, one or two participants still had to make their decision, they were explicitly (but anonymously) invited to hurry up; after at least two such warnings, the experiment was pushed forcibly to the next stage: the two resulting missing contributions (one for each condition) were recorded as 0 for the scope of other participants' payoffs, but the participant not making the decision would receive an individual payoff of 0 if that specific round was extracted for payment, and we consider such data as missing. Each round misses a number of observations comprised between 1 (round 6) and 5 (round 3).

Table 1
Comparison of experiments.

	Experiment 1	Experiment 2
Sample size	160	225
N. of patrols	31	44
Patrol sizes	3 (3), 4 (5), 5 (12), 6 (7), 7 (3), 8 (1)	3 (6), 4 (10), 5 (8), 6 (14), 7 (5), 8 (1)
Avg. patrol size	5.16	5.11
N. of sessions	7	17
Treatment dimensions	· *Divisible/Indivisible* · *Same/Other*	· *Divisible/Indivisible* · time (10 rounds)
Decisions per individual	$2 \times 2 = 4$	$2 \times 10 = 20$
Location	Summer camps in Trentino–Alto Adige	Online meetings with groups from Lazio
Group size	*Divisible condition*: 2 *Indivisible condition*: patrol size	Patrol size

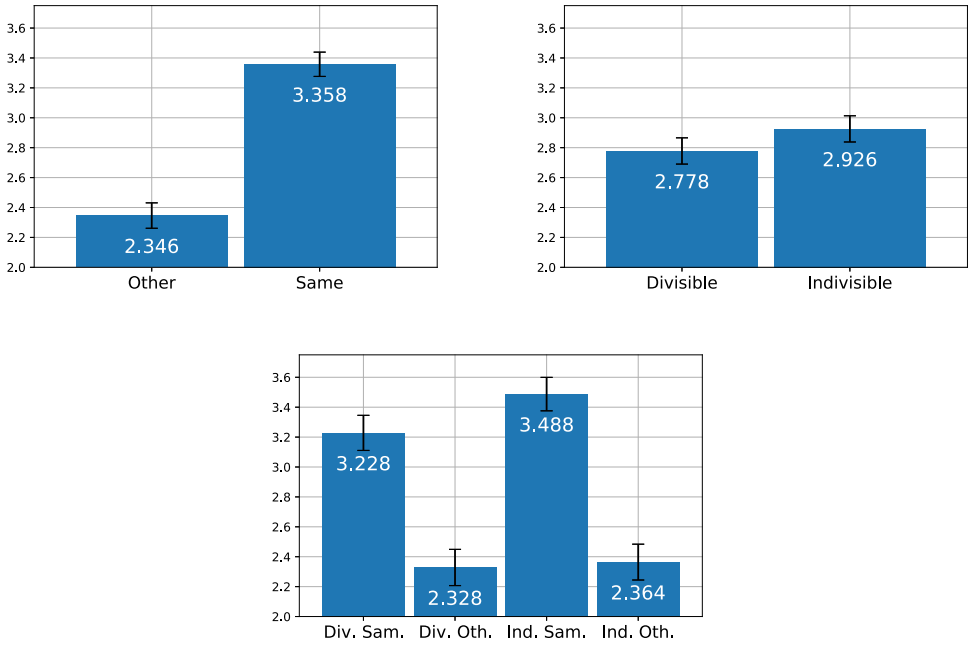


Fig. 1. First experiment: average individual contribution.
Note: bars indicate standard errors. Notice that the vertical scale does not include the entire range of possible contributions.

5. Results

5.1. First experiment

In the first experiment, the average of contributions across all treatment conditions is €2.85 (out of the initial endowment of €5). Fig. 1 presents an overview of average contributions within each treatment: in line with results generally found in the literature on PGG, they are in all cases significantly higher than the equilibrium prediction of zero. However, they show some treatment effects, with higher contributions featured in the *Indivisible* and in the *Same* conditions.

Table 2 shows the results of an OLS regression, with standard errors clustered at the patrol level. The main explanatory variables include two dummies for the two treatments (*Divisible* and *Other*), and their interaction. In order to control for the strong inter–subject heterogeneity, we introduce subject fixed effects in columns (1) and (2). In columns (3) and (4) we control instead for a range of covariates originating from the questionnaire administered at the end of the experiment, and including age, a dummy for being male, indicators of personality traits from the "Big Five" as in the revised Italian version of the Ten-Item Personality Inventory (Chiorri et al., 2015),[20] household size, and three measures of trust: trust in troop, in patrol, and generalized trust. This slightly reduces the sample size because of some missing answers in 15 questionnaires. The difference across the *Other-Same* treatment is

[20] In the Ten-Item Personality Inventory, possible answers to each question range from 1 (*completely disagree*) to 7 (*completely agree*); we measure each personality trait as the score of a question directly asking about the trait, minus the score of a question asking about the opposite trait; see Battiston et al. (2019) for more information.

Table 2
OLS results from first experiment.

	(1) contribution b/se	(2) contribution b/se	(3) contribution b/se	(4) contribution b/se
Divisible	–0.148	–0.259	–0.169	–0.288
	(0.132)	(0.202)	(0.144)	(0.222)
Other	–1.012***	–1.123***	–1.016***	–1.135***
	(0.086)	(0.147)	(0.109)	(0.164)
Divisible × Other		0.223		0.238
		(0.188)		(0.207)
Male			–0.022	–0.022
			(0.196)	(0.196)
Age			0.240***	0.240***
			(0.050)	(0.050)
Household size			–0.081**	–0.081**
			(0.024)	(0.025)
Generalized Trust			0.068	0.068
			(0.045)	(0.045)
Trust in Patrol			0.249**	0.249**
			(0.070)	(0.070)
Trust in Troop			–0.094	–0.094
			(0.086)	(0.086)
Extraversion (big 1)			–0.065	–0.065
			(0.033)	(0.033)
Agreableness (big 2)			0.002	0.002
			(0.061)	(0.061)
Conscientiousness (big 3)			–0.006	–0.006
			(0.039)	(0.039)
Neuroticism (big 4)			0.018	0.018
			(0.023)	(0.023)
Openness to experiences (big 5)			0.092***	0.092***
			(0.017)	(0.017)
Constant	3.432***	3.487***	–1.497	–1.437
	(0.100)	(0.131)	(1.266)	(1.304)
Individual fixed effects	✓	✓		
N.	640	640	584	584

Note: Standard errors clustered at the patrol level reported in brackets. * p<0.10, ** p<0.05, *** p<0.01

significant, including when we restrict to the *Divisible* or to the *Indivisible* conditions ($p < .01$ in all cases).[21] These findings can be summarized as follows:

Result 1. *As in standard PGGs, players contribute significantly more to an* Indivisible *public good when they are matched to a member of their community than when they are matched to strangers.*

The fact that this also holds in the *Divisible* condition confirms previous evidence that contributions to PGGs are larger in existing communities (see previous literature on this point in Section 2). Humans are, in fact, embedded in a social environment and have a social identity (Tajfel, 1982); which shapes how they see themselves as members of certain groups and not of others. This naturally induces a preferential treatment for, and hence a higher level of cooperation with, subjects' in–groups, relative to out–groups (Dasgupta, 2004; Tajfel et al., 1971; Ruffle and Sosis, 2006; Böhm and Rockenbach, 2013), with such cooperation also being more durable (Brewer, 1996) and sustainable (Berkman et al., 2015). Our experiment shows that in—group bias exists also when dealing with NOPGs; this constitutes a novel result in the existing experimental literature.

For what concerns the dimension of indivisibility of the common pool, contributions are on average €0.15 larger when the common pool is indivisible (see the upper right graph in Fig. 1) and in line with previous evidence (Alfano and Marwell, 1980), but the difference is not significant.

Still, there is a significant difference ($p = .000$) when we compare the NOPG obtained at the intersection of the *Same* and *Indivisible* conditions with the standard PGG at the intersection between the *Other* and *Divisible* conditions.

When looking at the heterogeneity in individual behavior, we find that all combinations of the four choices exhibit significant correlation within subjects ($p = .000$ for all 6 combinations, accounting for multiple hypothesis testing as proposed by Bonferroni, 1935).[22] The most interesting correlation, in light of the literature on external validity of social preference games, (Laury and Taylor, 2008; Voors et al., 2012; Galizzi and Navarro-Martinez, 2019), is arguably the one between *Indivisible/Same* and *Divisible/Other*,

[21] These results are confirmed by a non–parametric Wilcoxon signed–rank test of the null hypothesis that individual contributions in the *Indivisible/Other* and *Indivisible/Same* conditions come from the same distribution ($p = 0.000$).

[22] The possible unordered pairs of the 4 variables observed at the individual level are indeed $\frac{4 \cdot (4-1)}{2} = 6$.

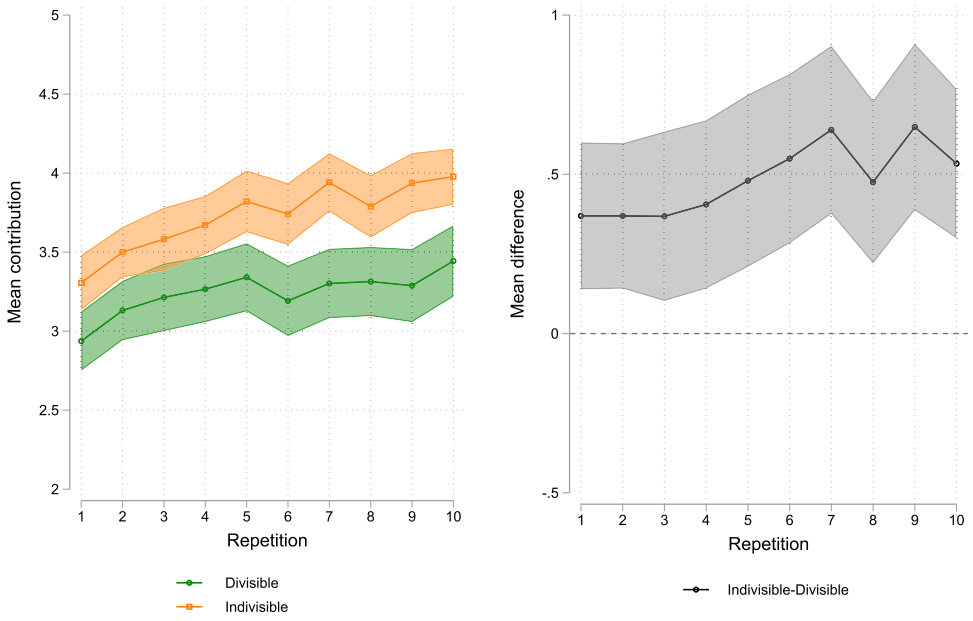


Fig. 2. Second experiment: average individual contribution over rounds.
Note: Left: comparison of divisible and indivisible condition; right: difference between the two. Shaded areas denote 95 % confidence intervals.

that is, between a NOPG, and a PGG. This correlation is 0.35,[23] which is quite large when compared to results found by Galizzi and Navarro-Martinez (2019) both in a meta–analysis, where the average correlation is 0.14 over a range of different lab and field designs (0.27 if restricting to significant correlations) and in their novel experimental data, where the correlation between decision in a PGG and in five different field behaviors (including donations to charities) is always lower than 0.15, and never statistically significant. This finding leads us to formulate the following result.

Result 2. *Individual contributions in the PGG correlate significantly with contributions in the NOPG.*

Unlike earlier studies, our design keeps the game's parameters and strategic structure constant, altering only two elements: the good's indivisibility and the social relationship among participants. This choice likely explains the stronger PGG–NOPG correlation we observe. The weaker correlation reported in previous work may not stem from the public-good nature per se, but rather from changes in other dimensions — such as the game's strategic architecture or the decision context. When those factors are held constant, the standard PGG becomes a credible predictor of behavior toward a comparable naturally occurring public good, particularly in settings with small groups and similar contribution ranges.

Overall, our results suggest that standard PGG experiments may be over–pessimistic in predicting *average* contributions, but more effective than previously assumed in characterizing the *heterogeneity* in the individual propensity to contribute to public goods.

The non significant result in the indivisibility treatment might be due to the number of beneficiaries, N, being on average much larger in the *Indivisible* ($N = 5.16$ on average) than in the *Divisible* condition ($N = 2$), so that the individual share (of the divisible common pool, or of ownership of the shared good) is much smaller (see Section 3).

The design of the second experiment addresses this potential confound by matching the members of the patrol to the subjects playing the voluntary contribution mechanism and thus keeping the number of potential contributors and beneficiaries the same across the *Divisible* and *Indivisible* conditions. This requires sacrificing the *Same–Other* dimension.

5.2. Second experiment

Let us now turn to the second experiment, conducted with a different sample of scouts. As a brief reminder of the design (see the full description in Section 4), participants belonging to a patrol played both a divisible and an indivisible public goods game, following a within-subject design. The main differences from the first experiment were: (i) both the PGG and the NOPG were played with members of the same patrol; as a consequence, (ii) there was no Same/Other treatment manipulation; (iii) participants made ten repeated decisions instead of just one.

Fig. 2 and Table 3 present the results of the second experiment, considering both the contributions within each condition, and the difference between them.

[23] All other combinations of choices feature an even higher correlation, which is to be expected at least for combinations that change only one aspect at the time – either the divisibility of the good, or the membership in a same real group.

Table 3
OLS results from second experiment.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Contribution								Difference	
	Overall	Rep. 1	Rep. 10	Overall	Overall	Rep. 1	Rep. 10	Overall	Rep. 10 - rep. 1	
Divisible	–0.483***	–0.369**	–0.554***	–0.325***	–0.492***	–0.329***	–0.581***	–0.288***		
	(0.118)	(0.166)	(0.171)	(0.097)	(0.084)	(0.121)	(0.126)	(0.100)		
Repetition				0.065***				0.067***	0.029**	0.029**
				(0.011)				(0.011)	(0.014)	(0.014)
Divisible × Repetition				–0.029**				–0.037**		
				(0.014)				(0.015)		
Male					0.064	–0.094	0.170	0.065		–0.057
					(0.134)	(0.170)	(0.197)	(0.134)		(0.199)
Age					0.047	0.107**	0.078	0.047		0.138**
					(0.043)	(0.050)	(0.061)	(0.043)		(0.057)
Household size					–0.007	0.009	–0.073	–0.007		0.070
					(0.040)	(0.073)	(0.072)	(0.040)		(0.065)
Generalized Trust					–0.020	–0.016	0.021	–0.020		0.020
					(0.036)	(0.038)	(0.052)	(0.036)		(0.042)
Trust in Troop					0.043	0.007	–0.005	0.043		0.017
					(0.060)	(0.058)	(0.070)	(0.060)		(0.072)
Trust in Patrol					0.001	0.099*	–0.025	0.001		0.046
					(0.044)	(0.051)	(0.067)	(0.044)		(0.060)
Extraversion (big_1)					0.013	0.028	0.027	0.013		0.052
					(0.031)	(0.036)	(0.046)	(0.031)		(0.043)
Agreableness (big_2)					–0.011	0.034	–0.017	–0.011		–0.014
					(0.029)	(0.035)	(0.043)	(0.029)		(0.038)
Conscientiousness (big_3)					0.047	0.033	0.084*	0.048		–0.036
					(0.030)	(0.036)	(0.043)	(0.030)		(0.041)
Neuroticism (big_4)					–0.024	–0.050	–0.018	–0.024		–0.044
					(0.027)	(0.033)	(0.040)	(0.027)		(0.044)
Openness to experiences (big_5)					0.025	0.009	0.075**	0.025		0.012
					(0.025)	(0.029)	(0.037)	(0.025)		(0.034)
Patrol members					0.082	–0.004	0.039	0.081		0.069
					(0.062)	(0.067)	(0.112)	(0.062)		(0.100)
Individual fixed effects	✓	✓	✓							
N.	4464	444	448	4464	4207	421	425	4207	2232	2203

Note: OLS estimates with standard errors clustered at the patrol–level reported in brackets. * p<0.10, ** p<0.05, *** p<0.01

Contributions to the *Divisible* common pool are significantly lower than in the *Indivisible* condition, both when considering all rounds together (columns (1) and (5)), when focusing on the first round only (columns (2) and (6)), or the last (columns (3) and (7)).[24] A first main finding of this experiment comes from contributions in the first round, and can be summarized as follows:

Result 3. *Contributions to a NOPG are significantly larger than to a PGG.*

This result mirrors the (not statistically significant) evidence from the previous experiment, with the vital difference that any possible confounding effect due to the varying number of beneficiaries is now removed. The fact that the difference is significant already in the first round rules out the possibility that this is a mere result of group behavior (relying on past actions as a coordination device): it follows instead from the different individual propensity to contribute to two rather different type of goods.

Result 3 provides clean evidence on the value of η being larger than 1: this means that the *scale effects* more than compensate any reduction in utility due to *joint consumption*.

When looking at average contributions over repetitions (see Fig. 2, columns (4) and (8) of Table 3 and Table B.4 in Appendix B) we see that, as the experiment progresses, average contributions increase both in the *Indivisible* and *Divisible* conditions.[25] Results are robust to the exclusion of those who did not pass the initial test and to the re-balancing of the sample obtained by randomly removing the number of women needed to recreate gender equality. This allows us to reach one further conclusion:

Result 4. *Contributions in both the NOPG and in the PGG increase over rounds.*

This result confirms the prediction made in Section 3 that real groups should be less affected by the decay in contributions. Indeed, a stylized result of the literature is that contributions to PGGs tend to decrease over time, mainly due to the vanishing relevance of reputational concerns under the so-called "no future" hypothesis, i.e., the fact that artificial groups created in lab experiments will never meet again once the experiment is over (see Andreoni 1988 and discussion in the literature review). However, the patrols

[24] These results are confirmed by non–parametric Wilcoxon signed–rank tests that contributions in the two conditions come from the same distribution ($p = 0.000$ in the pooled sample, $p = 0.012$ for the first round, $p = 0.000$ for the last round).

[25] We can reject the null hypothesis that the sum of the coefficients of Repetition and Divisible × Repetition equals 0 in column (4) of Table 3 ($p = 0.010$) and in column (8) ($p = 0.017$).

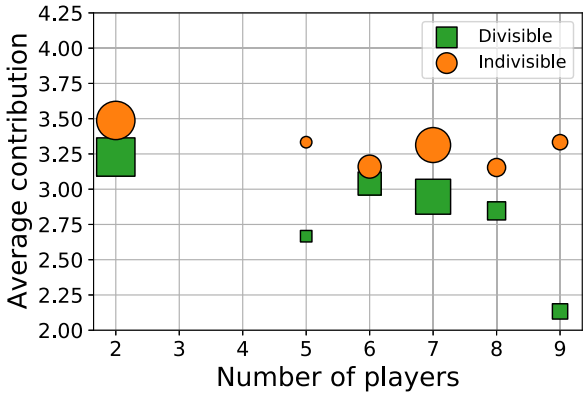


Fig. 3. Joint results from first and second experiment.
Note: comparison restricted to conditions that are directly comparable across experiments: *Same* from experiment 1 ($N = 2$) and first round from experiment 2 ($N > 2$). Marker sizes indicate numbers of observations.

we studied are organized communities where each patrol member enjoys their patrol's collective goods and will continue to do so for some time. Moreover, patrol members are used to interact on a regular basis and know they will continue to do so once the experiment is over[26] Findings from evolutionary game theory show that, especially when groups are small, longer–memory strategies make cooperation easier to evolve, by increasing the number of ways to stabilize cooperation even when the benefits for cooperation are low (Stewart and Plotkin, 2016).

The difference between contributions in the *Indivisible* and the *Divisible* conditions increases over time (see columns (9) and (10) of Table 3), confirming the last prediction made in Section 3.[27] Indeed, repetitions were expected to amplify the relative effect of the *Indivisible* condition; and this is precisely what we observe, with this condition showing higher contributions from the outset and a widening gap over time. This leads to the following result:

Result 5. *Contributions in the NOPG increase more than in the PGG.*

Finally, when looking at the two choices made by each individual in round 1 (before they could be influenced by the choices of peers), we obtain a statistically significant correlation of 0.24 ($p = .000$), supporting Result 2.

If we regress contributions in a given round over lagged contribution of other group members in the previous round, we obtain a positive but small (0.034) coefficient. While this could be interpreted as suggestive evidence of reciprocity, it is only marginally significant ($p = .092$ when estimating with fixed effects) and could be a spurious result of the complex interplay of group members over ten rounds: the effect disappears (coefficient -0.012, p =0.772) if we restrict the estimation to round 2.

5.3. Comparison of experiments

We run an exploratory comparison of the results from the two experiments, where feasible. Specifically, Fig. 3 summarizes the results from those conditions that can be matched between the two experiments: *Same* from experiment 1 (as experiment 2 did not have the *Other* condition) and first round from experiment 2 (as experiment 1 was a single shot). *Indivisible* conditions in the two experiments followed the same rules and envisaged no information on peers' behavior, but participants to the first round of experiment 2 knew other rounds would follow, and could potentially adopt strategic forward–looking behavior; moreover the *Indivisible* condition in the first experiment had 2 subjects playing together, but $N > 2$ beneficiaries of the public good. Despite these differences, we can observe in Fig. 3 that the effect of the *Indivisible* condition is remarkably consistent across both experiments and different patrol sizes.

Empirical evidence on the relation between group size and the level of contribution is notoriously ambiguous (see Pereda et al., 2019 for a recent assessment of the literature). In our *Divisible* condition, where the number of players corresponds to the number of beneficiaries as in traditional PGGs, this number appears to negatively affect contributions. However, in the *Indivisible* condition, more subjects do not seem to imply lower contributions. This represents suggestive evidence that the effect of group size on the contribution to public goods might be less detrimental in naturally occurring setting. If we augment the estimation in column (5) of Table 3 with an interaction term between *Divisible* and *Patrol members*, we get a coefficient which is negative, consistently with Fig. 3, but not significant (p =0.351).

[26] Indeed, increasing contributions were observed in other experiments where participants know each other well — Peters et al., 2004; Battiston and Gamba, 2020.

[27] These results are confirmed by a Wilcoxon signed–rank test comparing the average change in contribution, at the individual level, from the first 5 to the last 5 rounds between the two treatments ($p = 0.041$). The difference remains significant if we drop the two or four middle rounds ($p = 0.034$ and $p = 0.024$, respectively).

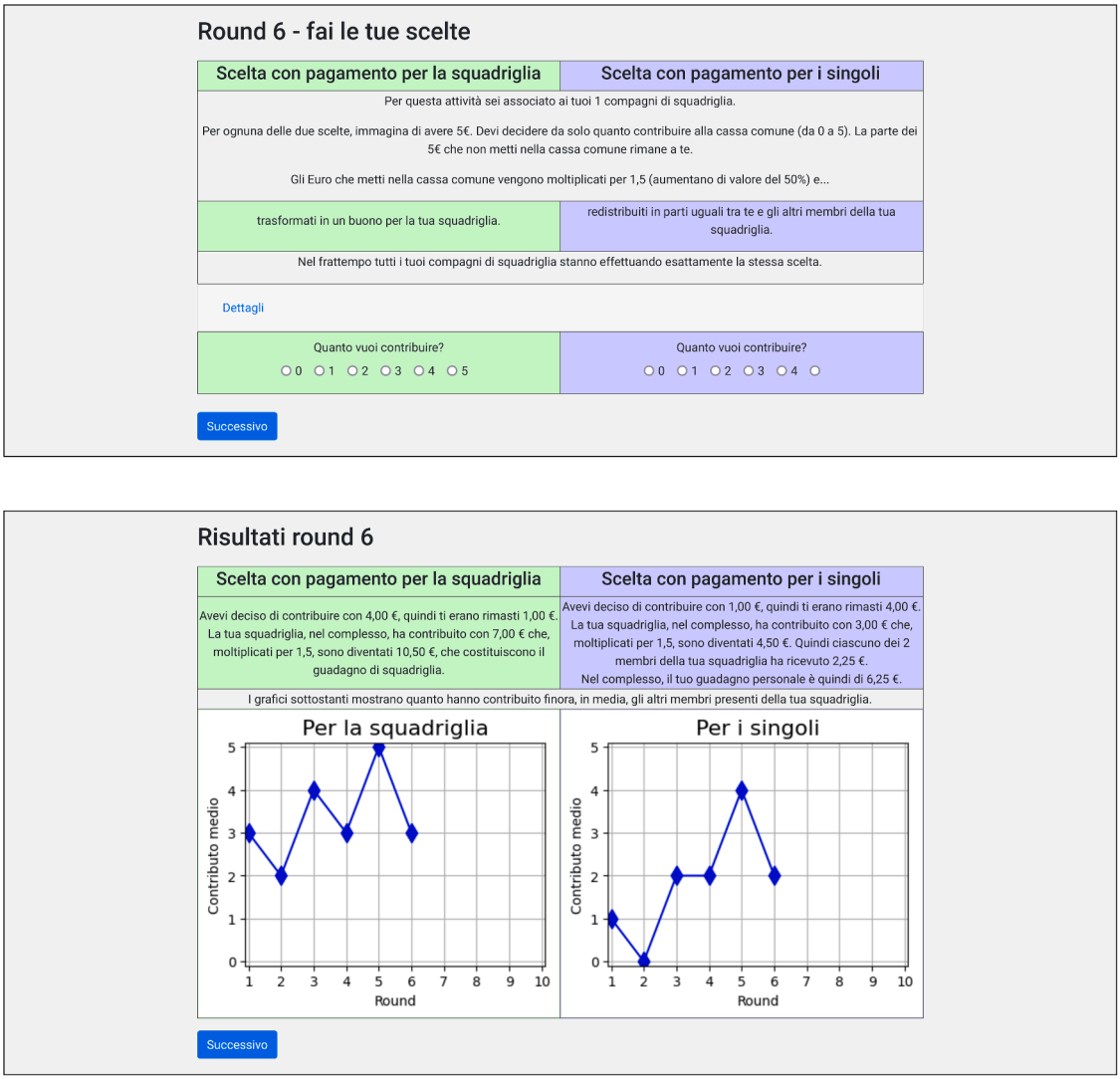


Fig. 4. Screenshots of the pages where the contribution choices were made (top) and where the round summary was subsequently shown (bottom). *Note:* the order of the two columns in each page was was randomized at the individual level, the color of each being consistent with that used in the instructions. By clicking on "*Dettagli*" ("More details"), subjects could expand a box with a reminder of the treatment–specific instructions.

In the analysis of each experiment, we have included regressions that control for a common set of covariates. Some of these, notably questionnaire answers referring to trust, could potentially act as mediators of the main treatment effect: we analyze this possibility in detail in Appendix B.1.

6. Discussion

Conducting these framed field experiments required a series of methodological choices, each one involving trade-offs. In this section, we outline both advantages and limitations that arise from those choices.

First, we selected the scout population because it is characterized by close-knit groups of similar size and characteristics, which own public goods. These characteristics have attracted the attention of other researchers before (see the literature review in Section 2). However, relying on such a unique and relatively homogeneous population necessarily narrows the external validity of our findings and may limit their applicability to other contexts. Future research should test whether these results hold in other communities, across different age ranges and group sizes. Moreover, it should investigate whether the effects of divisibility depend on the degree of connectedness and homogeneity among group members.

Second, while a one-shot online version of the first experiment or an in-person version of the second experiment could have provided additional information on the mode of delivery, practical constraints prevented us from implementing this. Specifically,

access to a broader and comparable sample of scout troops from other regions in Italy or further troops from the same regions (Trentino Alto-Adige and Lazio) was not feasible within the scope of our collaboration.

Third, in both experiments, we employ a within-subject design to collect multiple observations from the same individuals: (i) four choices spanning the Divisible/Indivisible and Same/Other conditions in the first experiment, and (ii) two choices across the Divisible/Indivisible conditions in the second experiment.

As in all within-subject experiments, each participant experiences multiple treatments, allowing causal effects to be inferred by observing how the same individual's behavior shifts across conditions. This approach offers three main advantages (Charness et al., 2012): it achieves internal validity without relying on random assignment; it delivers higher statistical power because each person serves as their own control; it mirrors our theoretical model, which tracks how a single agent responds to changing parameters and circumstances. Its chief drawback is that exposing subjects to several treatments can introduce confounds such as order effects, demand effects and altered frames of reference that ultimately complicate the external validity of the findings. While we took steps to mitigate order effects (see footnotes 13 and 14), we cannot entirely rule out the possibility of demand effects.

Still, even considering the case where the effect of divisibility (in experiment 2) may be inflated by demand effects, our results keep their qualitative relevance. On the one hand, because the direction of such effects is not obvious: both conditions offer clear benefits, making it unclear what participants might see as the "demanded" or more appropriate choice to please the experimenter.

On the other hand, because any demand effects that will lead a subject to favor the NOPG over the PGG are ultimately very similar to the kind of moral incentives that subjects face in real world decisions about public goods — comparing the benefits from full ownership of individual goods to those from partial ownership of shared goods.

A fourth point of discussion concerns the fact that, although our design is grounded in our model of joint consumption and scale effects, the observed outcomes may also be influenced by framing, salience, or other cognitive mechanisms. In particular, indivisibility may enhance the intuitive salience of the mutual nature of benefits, prompting participants to recognize that the good can only be obtained through joint effort-an effect that may be further reinforced by the within-subject design. These considerations caution against interpreting the results solely through a utility-based lens. Future research should aim to disentangle these influences-for instance, by comparing elicitation methods or manipulating salience independently of treatment content-to better understand the cognitive and motivational foundations of cooperation in public goods. Fifth, another fundamental design choice concerns the use of vouchers, that has both benefits and potential drawbacks. On the benefit side, the use of vouchers is justified by their high fungibility, almost equivalent to cash,[28] allowing them to be seamlessly integrated into a utility function and accurately reflecting participants' preferences and choices. Furthermore, the fact that scouts regularly buy both individual and patrol goods from the same scout store allows us to maintain comparable low transaction costs for the payment across the two treatment conditions; moreover, it guarantees that even small amounts can be of use, to help towards the purchase of more expensive goods.

However, vouchers also pose potential drawbacks: in principle, subjects could split collective vouchers for private purchases or pool individual vouchers to buy collective goods, blurring the treatment distinction. If this were to occur, the two conditions would become indistinguishable to the subjects, thereby undermining our intended treatment effect. Hence, the significance of the divisibility dimension (in our second experiment) is already a clear sign that individual and collective vouchers were perceived as intrinsically different (keeping fixed their overall value).

For what concerns the first experiment, fortunately we have concrete evidence of what was purchased using the vouchers assigned during the first experiment. The scout shop provided us with a list of items bought by members of each troop[29], allowing us to determine whether the items were paid for with individual or patrol vouchers.[30]

We classified each item as either a private good (e.g., personal clothing and accessories such as uniforms, hoodies, jackets, thermos, compasses, knives, headlamps, and mess kits) or a collective good (e.g., tent lamps, tent pegs, first aid kits, pole bags, camping stoves, and similar items. We also included in this category all official badges). Although the data records the number of items sold in each category to the different troops without indicating their economic value, it reveals a clear pattern: subjects from the two troops who were eventually paid according to the divisible condition and received only individual divisible vouchers purchased 63 private items and zero public items. In contrast, subjects from the four troops who were paid according to the indivisible condition-and thus received both patrol vouchers and individual vouchers-purchased 52 private items and 69 public items. The purchasing behavior observed in the first experiment is fully consistent with the intended premise that collective vouchers are used for collective goods, whereas individual vouchers are used for private goods.

A final point that deserves further discussion concerns the potential influence of authority figures on purchasing decisions, which may affect our intended treatment effect (e.g., parents when choosing private goods or patrol leaders when selecting collective goods). If external decision-making were to take place and if subjects anticipated this, it would render the treatment variations indistinguishable. In other words, why would participants care about the type of voucher if, ultimately, someone else makes the purchasing decision anyway? This potential issue would work conservatively against our main hypothesis, as it would obscure the distinction between individual and collective voucher conditions. However, the observed purchasing patterns clearly differ between

[28] The direct use of cash was ruled out because of the age of our subjects.

[29] We include the original excel file in the online repository https://osf.io/syuvn.

[30] Indeed, in the divisible condition, subjects receive individual vouchers, while in the indivisible condition, they receive both a collective patrol voucher and individual vouchers that cover the portion of their initial endowment they retain.

the two types of vouchers, as does the behavior of the participants during the experiment. This suggests that subjects perceived the treatments as fundamentally different, reinforcing the validity of our design.

7. Conclusions

The etymology of the word community goes back to two Latin words *cum* and *munis* that, combined, mean "to have rights and duties together". The great lesson of Nobel Prize winner Elion Ostrom is that public goods motivate the rise of communities and bond them together, inasmuch as communities regulate contributions to and rules of use of public goods.

If indivisibility and community membership are characterizing elements of NOPGs, they should also be central to the study of public goods in experimental contexts. However, standard PGG experiments run in lab settings focus on studying contributions to perfectly divisible common pools by subjects who belong to minimal groups, typically in absence of any sense of community outside the lab.

This paper adds to the growing literature on the external validity of Public Goods Games (PGGs) by examining a core assumption inherited from induced-value theory (Smith, 1976). Standard PGGs presume that private cash payoffs reproduce the strategic incentives of a naturally occurring public good (NOPG). Under this assumption, cash-maximizing participants will behave *as if* each token placed in the group account were an indivisible, collectively owned good embedded in their community — even though their final reward is private money. Our study can therefore be read as an empirical test of that *as if* proposition.

The treatment manipulation of our first experiment concerns the point of indivisibility and community membership, ultimately allowing a direct comparison of the private divisible good usually implemented in a traditional PGG and a more ecologically valid game involving an indivisible good.

Beyond confirming the insight from existing literature that group membership positively influences contributions to PGGs, our Result 1 extends this finding to NOPGs as well, and Result 2 shows that individual behavior is strongly correlated across the two cases. This result shows that when only divisibility is manipulated, PGGs have a good degree of external validity as a measure of relative willingness to contribute to the production of public goods. And yet, Result 3, shows that the propensity to contribute to indivisible NOPGs is significantly larger than to the divisible goods which characterize PGGs and which are typically studied in the laboratory.

With the second experiment, we also study a third important dimension of NOPGs: that they typically insist on communities that last over long time-horizons and require recurring investments to be maintained. Result 4 shows that the dynamics of contributions over time are *opposite* to those typically observed in PGGs: in our experiment, contributions within members of a same group *increase* significantly over time, even more so (Result 5) with indivisible goods. This is particularly surprising, as it reverses the well-established finding in both the experimental and theoretical literature that time and repetition typically undermine public goods provision. Instead, in our experiment where interactions are nested within a community with an ongoing future of regular cooperation around shared public goods, contributions increase over time.

In conclusion, and in light of the recent and growing literature on the external validity of PGGs, our findings provide a mixed picture. While contribution levels in the PGG correlate with those in the NOPG, contributions are higher when the public good is framed and delivered as a shared, indivisible good within a real group, and they also tend to endure over time. Speculating on why this might occur, one may recall the conclusion of Ramalingam et al. (2024), who argue that small experimental groups that are anonymous and lack communication opportunities (as in our PGGs) may behave more like large, impersonal groups, since intentions, norms, and expectations are difficult to signal and enforce. This cautions against extrapolating from *small, anonymous, no-communication* lab groups to *small, real-world* communities. Read in this light, our evidence — that contributions are higher and more persistent when the good is owned by the group and the group is real — suggests that insights from standard anonymous PGGs may be overly pessimistic for small-community public goods. This perspective may also help reinterpret the "disappointing" external-validity conclusions discussed in Section 2 and by Galizzi and Navarro-Martinez (2019). Future research may test more directly whether the tentative explanation advanced here indeed accounts for this divergence. Certainly our results highlight the importance of recognizing group *membership* and group *ownership* as central dimensions of real-world public goods — dimensions that typical experiments among anonymous strangers often overlook.

Data availability

The data generated and analyzed within the current study are available in the OSF repository at the URL https://osf.io/jkh5q/. Note that demographic variables have been omitted to safeguard the privacy of participants.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests:

Matteo Rizzolli reports financial support was provided by LUMSA University. If there are other authors, they declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

We will share raw data for both experiments and materials upon acceptance.

Code availability

The code to reproduce the results from both experiments will be made publicly available upon acceptance.

Supplementary material

Supplementary material associated with this article can be found in the online version at 10.1016/j.jebo.2025.107300

Appendix A. Experimental instructions and screenshots

A.1. First experiment

[Instructions are translated from Italian: they were included in the form distributed to each participant, and also read aloud. A "totem" is a traditional avatar for scouts, composed by an adjective and a name of animal or plant. Each participant was assigned randomly and anonymously a totem at the beginning of the session. In half of the sessions, the "Baden" activity corresponding to the indivisible condition, appeared before the "Powell" one (the divisible condition); in the other half the sessions, it was the opposite.]

In this second phase, you will take part in two activities: Baden and Powell. We will now read together the instructions of both activities. Afterwards, you will carry out the procedure individually, by following the instructions step by step. Only one of the two activities will be paid. We will decide which one by tossing a coin at the end of this phase: if head comes out, the Baden activity will be paid; if tail comes out, we will pay the Powell activity. Euros gained in this phase will be added to those from the previous phase. We will link earnings in this phase to those from the previous phase thanks to the totem which was assigned to you. It is very important that you write your assigned totem at the top of this sheet!

INSTRUCTIONS - BADEN

You have €5. You will have to independently decide how much to contribute to a common pool (between €0 and €5). The remaining of the €5 will be left to you. You have been paired randomly to another member of your troop - who may or may not be part of your patrol. He or she will also decide how much (between €0 and €5) to contribute to the common pool.

The euros put by you and the other participant in the common pool will be multiplied by 1.5 (that is, they increase in value by 50 %).

If you are both part of the same patrol, all euros in the common pool (multiplied by 1.5) will be paid to your patrol. If you and the other participant are part of two different patrols, the euros in the common pool (multiplied by 1.5) will be subdivided in equal parts between your patrols.

- **Your earnings** in this case are given by the euros that you kept for you.
- **The other participant's earnings** are given by the euros he or she kept for him/her.
- If you are both members of the **same patrol**, the patrol earns the euros in the common pool (multiplied by 1.5).
- f you are members of two **different patrols**, each patrol will earn half of the euros in the common pool (multiplied per 1.5).

Example: Astute Marmot was paired with Roman Pine. Astute Marmot contributes €4 to the common pool, while Roman Pine contributes €2. Astute Marmot hence keeps €5-4 = €1, Roman Pine keeps €5-2 = €3. In the common pool there are €6 which, after being multiplied by 1.5, become €9. Hence, if both are members of the same patrol, their patrol receives €9, while if they are members of two different patrols, each of the two patrols receives €4.50.

What happens if Astute Marmot contributes €0 and Roman Pine €5? What happens if Astute Marmot contributes €2.5 and Roman Pine €2.5?

Now please make your choice in each of the following cases:

["INSTRUCTIONS - POWELL" read as above with the following differences: the sentence in bold "If you are both part […] between your patrols." was replaced with]

Euros in the common pool (multiplied by 1.5) will be subdivided in equal parts between you and the other participant.

[And the bullet points that followed were:]

	If the other participant is a member of **your patrol**	If the other participant is a member of a **different patrol**
Euros in the common pool		
Euros you keep for yourself		
The column must sum up to €5	€5	€5

- **Your earnings** in this case are given by the euros that you kept for you, plus half of those in the common pool (multiplied by 1.5).
- **The other participant's earnings** in this case are given by the euros he or she kept for him/her, plus half of those in the common pool (multiplied by 1.5).

[The same example as above was proposed, with its outcome modified accordingly]

A.2. Second experiment

[Instructions are translated from Italian: they were show on screen, and also read aloud. Each participant was assigned randomly and anonymously the name of a city via participation codes distributed in advance. Fig. 4 diplays two screenshots of the experimental interface.]

Welcome to this activity

The activity is organized by some researchers in collaboration with the Scout Research Center. All members of your troop have been invited to this activity.

The code you received is personal and gives you access to the activity. The activity guarantees your anonymity. This means we will not be able to reconstruct who made what decision.

First, we will read the activity instructions together. You will also see these instructions on your screen throughout the activity. If, while reading the instructions, there is anything you are unclear about or you have any doubts, you can write to us in chat - only we will be able to read what you write.

After we will have finished reading the instructions, there will be a simulation. That is, you will be presented with an example and asked to answer some questions. If you have any doubts about how to answer, you can always write to us in chat.

The actual activity will appear on your screen when you have all answered the simulation questions correctly. The activity consists of 10 steps called *rounds*. In each round, you will have to make two decisions. When you have entered your choice, click "Next" to go to a summary screen for the round and then to the next round. The instructions are the same for each round. However, you can re-read them whenever you want, as they will always appear on your screen.

At the end of the activity, you will be asked to complete a questionnaire. As with your choices in the activity, your anonymity is guaranteed — we will not know who gave what answers.

To thank you for your participation, we will distribute vouchers to spend at the FSE Scouting store (purchases can also be made online). The vouchers may be assigned to the patrol or to the individual. Their value will depend on the choices you make during the activity. Since there are ten rounds and you will make two decisions in each round, we will use an online site to randomly draw the actual payment. There will be two draws: the first will determine which of the two choices will be considered, while the second will determine which of the ten rounds will be considered.

To conclude, there will be a time of debriefing, where we will discuss the activity that has just taken place.

In total, the study will last about two hours.

Description of the activity

This activity consists of two choices.

You are associated with your patrol mates, but you will make each choice individually.

You have €5. You have to decide how much to contribute to a common pot (from 0 to 5). The part of the €5 you do not put into the common pot stays with you.

The Euros you put into the common cash box are multiplied by 1.5 (they increase in value by 50 %). After that, what happens depends on the type of choice.

In the **choice with payment for the patrol**, the contents of the common pot – multiplied by 1.5 – is turned into a voucher for your patrol.

In the **choice with payment for individuals**, the contents of the common pot – multiplied by 1.5 – are redistributed equally between you and the other members of your patrol.

Meanwhile, all your patrol mates will face exactly the same decisions as you. Your final earnings then will be given:

in the **choice with payment for the patrol**, by

- The Euros you kept for yourself...

... while your patrol's earnings are given by:

- the Euros from the common pot (multiplied by 1.5)

in the **choice with payment for individuals**, by

- The Euros you kept for yourself...
- ... plus the Euros you receive through the choices of other patrol members. Indeed, the Euros put into the common pot by all patrol members will be multiplied by 1.5 and divided equally among all members (including absentees).

After each round, you will see a summary screen showing average contributions of your patrol mates.

Example

The patrol consists of Marrakech, Panama and Tallinn. Marrakech contributes €2 to the common pot, Panama contributes €4 and Tallinn gives €2. Marrakech thus keeps for herself 5-2 = €3, Panama €1 and Tallinn €3. In the common pot there are €8, which after multiplication by 1.5 become €12.

Appendix B. Additional material

Table B.4
Second experiment: round–specific results.

Round	Condition	Obs.	Average	Std. Error	Std. Deviation
Overall	indivisible	2232	3.727	0.030	1.429
	divisible	2232	3.244	0.034	1.625
	difference	2232	0.483***	0.041	1.929
1	indivisible	222	3.306	0.089	1.324
	divisible	222	2.937	0.095	1.422
	difference	222	0.369***	0.117	1.749
2	indivisible	223	3.507	0.081	1.215
	divisible	223	3.135	0.096	1.427
	difference	223	0.372***	0.115	1.724
3	indivisible	221	3.588	0.101	1.504
	divisible	221	3.222	0.108	1.613
	difference	221	0.367***	0.134	1.999
4	indivisible	223	3.677	0.094	1.403
	divisible	223	3.274	0.106	1.583
	difference	223	0.404***	0.134	1.995
5	indivisible	224	3.826	0.099	1.483
	divisible	224	3.348	0.109	1.639
	difference	224	0.478***	0.137	2.046
6	indivisible	225	3.733	0.100	1.494
	divisible	225	3.182	0.114	1.703
	difference	225	0.551***	0.134	2.017
7	indivisible	223	3.942	0.094	1.402
	divisible	223	3.305	0.111	1.662
	difference	223	0.637***	0.134	1.995
8	indivisible	224	3.772	0.102	1.520
	divisible	224	3.317	0.111	1.660
	difference	224	0.455***	0.131	1.956
9	indivisible	223	3.928	0.097	1.447
	divisible	223	3.291	0.117	1.750
	difference	223	0.637***	0.133	1.984
10	indivisible	224	3.982	0.090	1.353
	divisible	224	3.429	0.116	1.732
	difference	224	0.554***	0.121	1.804

Note: Stars show the degree of significance calculated using a paired *t*-test. * p<0.10, ** p<0.05, *** p<0.01

B.1. Trust as a potential mediator of treatment effects

In our main specifications in Tables 2 and 3, we include a comprehensive set of individual-level control variables, including socio-demographic characteristics and personality traits. However, some of these variables-particularly trust in one's troop or patrol-may plausibly lie on the causal pathway between treatment assignment and contributions. That is, they may act as mediators rather than confounders. This concern is consistent with experimental evidence suggesting that trust and reciprocity are not fixed traits but are shaped by social interactions and institutional environments. For instance, Fehr et al. (2002) show that cooperative institutions can activate strong reciprocity norms, increasing trust and prosocial behavior even in anonymous, one-shot settings. In our context, the treatment manipulation-by altering the perceived structure or framing of the interaction-may influence group-specific trust, which in

Table B.5
OLS Results: Both Experiments - Assessing the Role of Trust as Possible Mediators.

	(1)	(2)	(3)	(4)
	Dependent variable: contribution			
	b/se	b/se	b/se	b/se
	First Experiment			
Divisible	-0.288	-0.288	-0.288	-0.288
	(0.222)	(0.221)	(0.221)	(0.221)
Other	-1.135***	-1.135***	-1.135***	-1.135***
	(0.164)	(0.164)	(0.164)	(0.164)
Divisible × Other	0.238	0.238	0.238	0.238
	(0.207)	(0.207)	(0.207)	(0.207)
Generalized trust	0.068		0.054	0.078*
	(0.045)		(0.040)	(0.034)
Trust in troop	-0.094	-0.072		0.056
	(0.086)	(0.080)		(0.066)
Trust in patrol	0.249**	0.257**	0.184*	
	(0.070)	(0.074)	(0.076)	
Individual controls	✓	✓	✓	✓
N.	584	584	584	584
	Second Experiment			
Divisible	-0.288***	-0.289***	-0.287***	-0.289***
	(0.098)	(0.098)	(0.098)	(0.098)
Repetition	0.067***	0.067***	0.067***	0.067***
	(0.011)	(0.011)	(0.011)	(0.011)
Divisible × Repetition	-0.029*	-0.029*	-0.029*	-0.029*
	(0.015)	(0.015)	(0.015)	(0.015)
Generalized trust	-0.024		-0.020	-0.023
	(0.036)		(0.036)	(0.036)
Trust in troop	0.042	0.034		0.053
	(0.062)	(0.060)		(0.050)
Trust in patrol	0.015	0.009	0.043	
	(0.045)	(0.044)	(0.041)	
Individual controls	✓	✓	✓	✓
N.	4207	4207	4207	4207

Note: OLS estimates based on a consistent sample of participants with complete data on all covariates. Individual controls include age, gender, household size, Big Five personality traits, and number of partners. The three trust measures-generalized trust, trust in troop, and trust in patrol-are introduced selectively across columns. Standard errors clustered at the patrol level are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

turn could affect contributions. Including such variables as controls could therefore absorb part of the treatment effect. To assess this, Table B.5 reports alternative specifications based on a consistent sample of participants with complete data on all covariates. Columns 2-4 progressively exclude trust-related covariates, enabling us to test the robustness of our main results to potential mediation. The estimated treatment effects remain highly stable across models in both experiments, indicating that our main findings are not driven by mediation through reported trust levels.

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